PHYSICS TODAY VOL 19 NO 2

The Electron Dipole Moment —a case history

The anomalous magnetic moment of the electron is of great significance in the study of elementary particles and quantum electrodynamics. The history of its discovery is in large part the story of a technique—the use of molecular beams—which was not invented for the specific purpose but which did, by permitting a discrepancy to appear where none was expected, answer an unasked question.

by Polykarp Kusch

THE ELECTRON is one of the fundamental particles in the universe and is likely to remain one. It is as abundant as any other particle with the possible exception of the neutrino. There may be more neutrinos around, but I am not expert on that question. The electron has a definite charge and a definite rest mass.

Sir J. J. Thomson, one of the great experimental physicists of his time, is given credit for the discovery of the electron in 1897. But no matter what else Thomson did, he surely did not discover the electron. He did demonstrate through an ingenious experiment that whatever carries the negative charge in a current of electricity has a constant and, from the point of view of his time, a surprisingly large ratio of charge to mass. Although Thomson knew the approximate charge on the electron, he had no convincing evidence that the charge alone was constant.

Polykarp Kusch, a professor of physics at Columbia University, was co-winner of the 1955 Nobel Prize in physics for his contributions to the work whose history he describes. As a coda to his description, he remarked, when this article was already in press, that ten years later the prize was given to Feynmann, Schwinger and Tomonaga for work that contains a complete theoretical treatment of the anomalous moment.

It took further work, particularly that of Robert Millikan[1] in 1911, to demonstrate that the charge on the electron is separately constant and that the mass is therefore also constant. Only then did the electron become a well-defined entity. Ultimately, with the advent of the Bohr theory of the hydrogen atom, a very high precision was obtained for the result that the charge and mass are separately constant if their ratio is constant.

The next big step in the description of the properties of the electron was made by S. A. Goudsmit and G. E. Uhlenbeck[2] in 1925. They made two postulates: (a) the electron has an intrinsic angular momentum of $h/4\pi$; (b) there is a magnetic dipole moment equal to $eh/4\pi mc$. This quantity is called the Bohr magneton and is represented by μ_0. If we call the spin magnetic dipole moment of the electron μ_s, then $\mu_s = \mu_0$.

The postulates of Goudsmit and Uhlenbeck were profoundly important in the growing knowledge of physics on an atomic scale. They were proposed on the basis of a critical analysis of the optical spectra of atoms and in particular of the Zeeman effect. Like all the really important statements in physics, they were not consequences of a preëxisting body of theory. They were offered as ad hoc assumptions that were consistent with known data and subject to confirmation by further experiment, especially experiment designed to test the assumptions. The postulates are among the truly

great statements of physics; Goudsmit and Uhlenbeck did more for our knowledge of the electron than anyone else since, and probably before, their time. In 1928 P. A. M. Dirac formulated the relativistically invariant quantum mechanics; both of the statements of Goudsmit and Uhlenbeck were shown to be consequences of the Dirac quantum mechanics. They were no longer ad hoc assumptions but part of a powerful, intellectual construct whose validity was brilliantly confirmed by a large range of experimental evidence.

The important physical question, "Is μ_s precisely equal to μ_0?", was not wholly ignored. In 1925 Ernst Back and Alfred Landé measured μ_s and came to the conclusion that it was equal to μ_0 within an experimental uncertainty of at least one part in a thousand. According to what we know now, there was then no chance of discovering the discrepancy between μ_s and μ_0 with that kind of precision. Landé made important contributions to quantum theory and his name is most frequently used in the words *Landé-g-factor*, the negative ratio of a magnetic dipole moment in units of μ_0 to the angular momentum in units of $h/2\pi$. For the electron it is 2, or was until the discovery of the anomalous moment of the electron.

In 1934, Kinsler[3] made a study of the Zeeman effect in neon in both the $^3P_1$ and the $^1P_1$ states. Neon has a Z of 10 and the two isotopes of atomic mass numbers 20 and 22 account for 99.74 percent of natural neon. There is thus no hyperfine structure and the observation of the Zeeman effect is much simplified. Both of the states arise from the same electronic configuration $1s^2\ 2s^2\ 2p^5\ 3s$ and are the only states with a J of 1 from this configuration. If Russell-Saunders coupling is assumed, then for $^3P_1$, $g_J = g_L/2 + g_S/2$. For $^1P_1$, $g_J = g_L$. Naively you might suppose that g_J could be measured for both states and that $2g_J(^3P_1) - g_J(^1P_1) = g_S$.

This is fine if we *know* that we have Russell-Saunders or LS coupling. We can, however, do better. There is a sum rule which states that the sum of the g_J values of all states of the same J from a specified electronic configuration is independent of the coupling scheme. Accordingly,

$$g_J(^3P_1) + g_J(^1P_1) = 3g_L/2 + g_S/2.$$

Kinsler obtained, for the sum, 2.5017 ± 0.0016. In view of the assigned experimental uncertainty, the result was considered to be in agreement with the value of 2.5000 predicted on the basis of the accepted g values of the electron. On the basis of present day knowledge, or the authoritative canon of contemporary physics, the sum is expected to be 2.5012.

But outside of these attempts which have no very great place in the literature, the assertions of Goudsmit and Uhlenbeck were generally accepted as gospel truth, especially since they were also obtained from the Dirac equation. Perhaps the assertions *were* gospel truth. Physics knows no criterion for truth other than observable reality. There just were no observed phenomena before the second World War that suggested the questioning of the assertions.

It was not until 1948 that the discrepancy now described as the anomalous magnetic moment of the electron was first authoritatively demonstrated. It turns out that experimental technique as late as 1947 or 1948 was primitive—appallingly primitive—by present day standards. But the result that the magnetic dipole moment of the electron is anomalous could have been obtained long before 1947 or 1948 if someone had just asked the question. Available techniques in, say, 1939 could have been adapted to the measurement of the spin magnetic moment of the electron. Any really new and striking result in science requires experimental skill and imagination of the highest order, as well as insight, enthusiasm, energy and a lot of other qualities. But the asking of a good question is almost always a necessary antecedent to the process of productive experimentation.

Development of molecular beams

I will now discuss briefly the development of an experimental method, generally described as the method of molecular beams, that led to the discovery of the anomalous dipole moment of the electron. The molecular beam method allows one, with a certain amount of ingenuity, to examine some properties of atoms and molecules that are essentially free of interactions with other atoms or molecules. With all the striking improvements in the techniqques and methodology of experimental physics over the last fifty years it would be difficult to find another rather general procedure that allows cleaner investigation of atoms and molecules.

The molecular-beam method (the phrase is intended to include atomic beams) is not only a technique but was at one time something of a mystique. You had to be especially anointed to cope with it. The technique had devotees and the devotees were members of a sort of cult; if they never got around to adopting an old school tie it was because they were too busy. With the progress of physics there are alternative techniques for doing what was once in the domain of the molecular-beam technique, and the cult is dying out.

In 1911 L. Dunoyer[4] described the production of

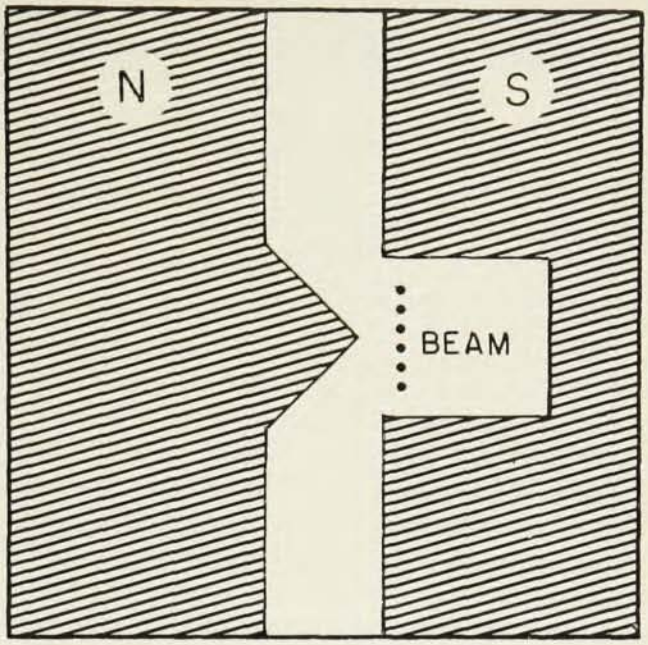
a beam of atoms. It almost certainly was not a trivial feat by the standards of the day; the mercury diffusion pump was not, after all, invented until 1915. Dunoyer's experiments demonstrated that in a vacuum atoms move in straight lines, because extremely sharp shadows were cast by objects placed in the path of atoms effusing from a small orifice.

The most celebrated of the early experiments was the famous one by Otto Stern and Walter Gerlach[5] in 1921. It gave striking confirmation of the physical reality of the quantization of angular momentum along a magnetic field, an essential part of the prevailing quantum theory. According to that theory the angular momentum could not assume an arbitrary orientation along a magnetic field; it could assume only those orientations in which the component of the angular momentum along the field is an integral multiple of $h/2\pi$.

In the Stern-Gerlach experiment a beam of silver atoms issues from a slit in an oven and is further collimated by a slit at some considerable distance from the oven. The ribbon of silver atoms then passes through an inhomogeneous magnetic field produced by a pair of pole pieces approximately as shown in figure 1. In spite of the inhomogeneity of the field, the general direction of the field, is from left to right.

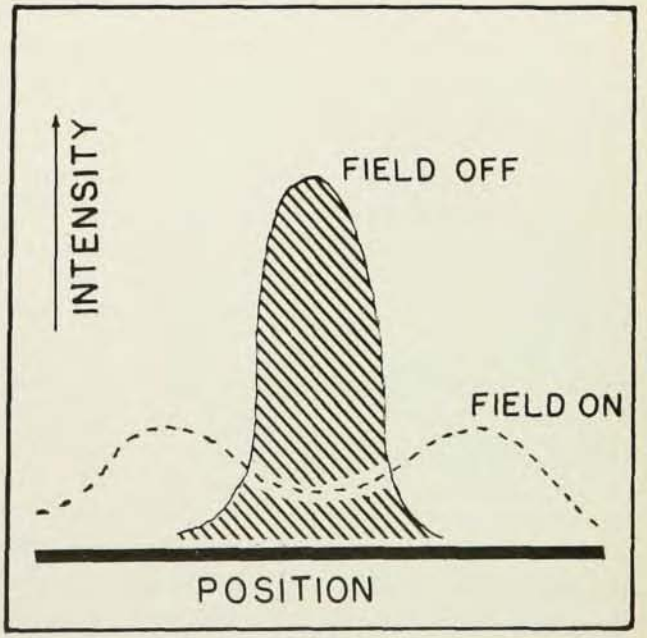
Consider an atom in which the electron has an orbital angular momentum of just $h/2\pi$. By our rules, this can be either parallel or antiparallel to the field. By a special rule of the quantum mechanics of the time, the position perpendicular to the field was excluded when the component of the angular momentum was zero times $h/2\pi$. The atoms would then experience a force either to the left or to the right. The force would be of the same magnitude for all atoms but would be applied for different lengths of time to atoms of different velocity. The net effect was that the particles deposited on a screen beyond the magnet had an intensity distribution as shown in figure 2. If there were no space quantization and all orientations of the angular momentum with respect to the field were possible, the *field-on* curve would be broader than the *field-off* curve but would not show maxima. The agreement with expectation was perfect.

Actually Stern and Gerlach did not do quite what they believed they had done. We now call the ground state of silver a $^2S_{1/2}$ state. The orbital angular momentum is zero and the total angular momentum is the spin angular momentum of the single electron outside of a closed shell. We would now say that the two components of the beam are described by the quantization of the spin angular momentum of the electron along the field—that is, $m_s = \pm 1/2$.



POLE PIECES FOR AN INHOMOGENEOUS MAGNETIC FIELD. From the Stern-Gerlach experiment. —FIG. 1.

Although the spin magnetic moment had not yet been postulated and the experiment was interpreted with the help of a faulty model, it was a great experiment. A magnetic dipole moment did have only two possible components along a field and the evidence for the physical reality of space quantization became direct. It should be noted that the Stern-Gerlach experiment had a



INTENSITY DISTRIBUTION OF PARTICLES in the Stern-Gerlach experiment. —FIG. 2.

feature common to many atomic-beam experiments. It dealt with a property of a single electronic state, the ground state. On the other hand, the very large body of spectroscopic data that provided the guideposts for an evolving quantum theory dealt with the frequency of transition between two widely separated electronic states.

In 1927 a clever young man, Isidore I. Rabi, went to Stern's laboratory. After two years in Europe, Rabi joined the faculty of Columbia University and started a program in molecular-beam research. The work was successful from the outset and much first-rate work was done. I will not describe the work except to comment that it contributed to an insight into the behavior of atoms.

The great idea came to Rabi in 1937. His discovery of the molecular-beam magnetic-resonance method is to some degree the parent of almost every technique of modern spectroscopy—the nuclear magnetic resonance method, microwave spectroscopy, optical pumping, paramagnetic resonance and still other spectroscopic methods. Rabi's essentially spectroscopic method was the first to operate at frequencies that could be generated by electromagnetic devices. All previous spectroscopies dealt with transitions at optical frequencies or wavelengths, where I include those wavelengths that are handled by typically optical devices.

At optical frequencies the overwhelming term in the energy of transition is always an electrostatic one arising from the energy difference between two electronic states, and it is not really very interesting. The things of profound interest in 1937 were the splitting of the ground state of an atom or of a molecule and the further splitting of each component level in a magnetic field. The new method revealed details of energy levels wholly unaccessible to previous methods of observation.

Such splittings had been observed as minute splittings of the spectral lines that result from transitions between two electronic states of atoms. Indeed the Zeeman effect and hyperfine structure were discovered in optical spectra, but the splittings were always minute and many effects that are now commonplace had not been even marginally observed. As an example, the Lamb shift in hydrogen of about 1000 Mc/sec or 1/30 cm^{-1} had not been observed in optical spectra.

There is nothing in the set of ideas involved in the molecular-beam magnetic-resonance method, or in the experimental techniques involved, that would not have allowed discovery and application of the method about eight years earlier. First of all it is necessary to have a vacuum good enough so that the mean free path of molecules is of the order of a meter or more. But long beams had been produced since the twenties and scattering had not been an insuperable problem. With painstaking care about cleanliness and a careful use of pumps, eminently respectable vacuums could be achieved. The inhomogeneous deflecting fields in the Rabi device were considerably more sophisticated than those of Stern but they could have been invented a half century before Rabi devised the first molecular-beam spectrometer.[6]

An essential component of any atomic- or molecular-beam apparatus is a detector. A large range of detectors has been used, but only since 1929, when J. B. Taylor[7] developed the surface-ionization detector, has it been possible rapidly, efficiently and selectively to detect beams of alkali atoms or of molecules that contain an alkali atom.

The development of the hot-wire detector for atoms of low ionization potential was extremely important. Its principle is, briefly, that if an atom strikes a hot surface with a work function greater than the ionization potential of the atom, the atom becomes ionized. Therefore a flux of atoms to the detector may yield an ion current upon application of a suitable potential difference between the hot surface and a collector. The work function of clean tungsten is about 4.50 eV, so that cesium, rubidium and potassium (with lower ionization potentials) can be readily detected. If the tungsten surface is oxidized, its work function increases; beams of thallium (ionization potential 6.1 eV) have thus been successfully detected. The important point is that the detector is wholly insensitive to atoms and molecules that remain in even an excellent vacuum system. The detector is also fast and, for any realistic beam intensity, linear. The application of modern technique allows the selective detection of any atomic species, but the process is much more complicated and much less efficient. Without the Taylor detector the whole subject would have developed much less rapidly.

Finally, electronic oscillators to generate power at frequencies up to many megacycles per second were clearly available. The point is that the method was not invented earlier because no one thought of inventing it. Like many inventions, it seems obvious once it has been invented. Not only was it necessary to put together all the ingredients to devise a workable spectroscopic technique, but it was necessary to conjure up the idea of reflecting about a new spectroscopic technique.

Any spectroscopic method, including the molecular-beam magnetic-resonance method is simply a procedure for answering the question, "Has or

CHARGED PARTICLES

Extending the capabilities of research equipment

Results from Tandem Research Program

The Tandem Research Group has made notable progress in the past year. Significant experimental results from the program are:

1. 250 mA high-brightness positive ion beam from an expanded-plasma source operating at 38 kv.

2. 270 μA analyzed beam of H_1^+ ions out of the Research Tandem with 320 μA H^- injection and water-vapor stripping.

3. 2.0 μA analyzed dc beam of He^- ions. The previous maximum current routinely available has been 0.1 μA with the EN source.

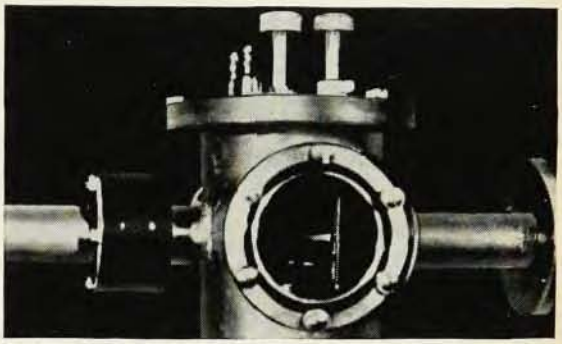
Doubly Charged Helium Ions

Components are now available for converting 3, 4 and 5 MeV machines to produce He^{++} ions at higher energies. Specifications: 30 μA at 5.0 MeV; 10 μA at 7.0 MeV; 5 μA at 10.3 MeV. More than double this current performance has been demonstrated but with some loss in stability and reliability. Multiple-charge states (2, 3 and 4) of neon, oxygen and nitrogen have also been produced with the new kit installed in a 3 MeV Van de Graaff. Beam energies from 5.04 MeV to 9.8 MeV and beam currents from 0.1 to 10 μA were observed. For details on the new HE^{++} kit and experimental results, write for Technical Note #13.

Optical Spectroscopy of Excited Atomic States

When an energetic beam of ions is passed through a thin foil, the charge state of the ion may change, either up or down. The emitted particles may be left in states of electronic excitation from which visible light is subsequently emitted during de-excitation. The emitted light spectrum is characteristic of the excited ion. When particle beams of approximately 0.4 μA or more are used, the light is sufficiently intense for spectroscopic analysis.

The refinement and application of this technique promises to be of major importance in the theory of atomic structure, in measuring hot plasma temperatures, and in acting for the means of energy loss in fast fission fragments in an absorber. Perhaps most importantly, it will help determine the relative abundance of the elements in the sun and other stars, which is the basis for theory of stellar evolution, the origin of the chemical elements, the age of astronomical objects and the nature of the stellar energy. For further details, ask for Technical Note #10.

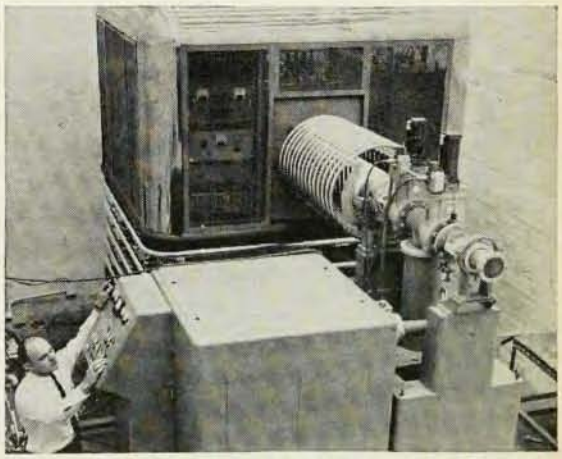


A nitrogen beam, 0.8 μA at 2 MeV, passes from right to left through a carbon foil approximately 9 μg/cm² thick.

Intense Ion Beams at 500 kv

The ICT-500 keV positive ion accelerator now being built by High Voltage Engineering operates at energies from 100 to 500 keV dc and pulsed. In performance tests, the machine has produced analyzed ion beam currents from 4 mA at 100 keV to 10 mA from 300 to 500 keV. 10 mA dc positive ion beam currents of H^1, H^2, and D^1 have been produced at a target located 6 feet from the end of the acceleration tube. Beam diameter is 15 millimeters maximum for all particles over the entire energy range. Previous experience with a similar machine of 300 keV maximum energy showed 15 mA of d_2^+ and a 3 centimeter beam diameter. The ICT-500 positive ion accelerator is designed for dc and pulsed operation in the nanosecond and microsecond range with a minimum pulse length of 2 nsec. at a repetition rate of 2.5 Mc/s. Pulse content is 1 mA protons and 0.7 mA deutrons.

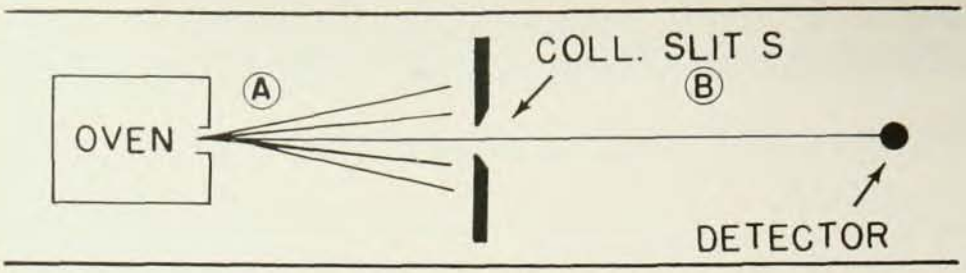
The particle source utilized with the ICT-500 positive ion accelerator is an expanded plasma type which has produced 70 mA total beam at 500 kv.



The high-brightness, intense ion beam produced by the ICT-500 accelerator is eminently suited for laboratory production of 14 MeV neutrons for cross-section measurements, dosimetry studies, weapons-effect simulation and special low-density target experiments.

For detailed information, write to Technical Sales, High Voltage Engineering Corp., Burlington, Mass. or HVE (Europa) N. V. Amersfoort, The Netherlands. Subsidiaries: Electronized Chemicals Corporation, Ion Physics Corporation.

HIGH VOLTAGE ENGINEERING

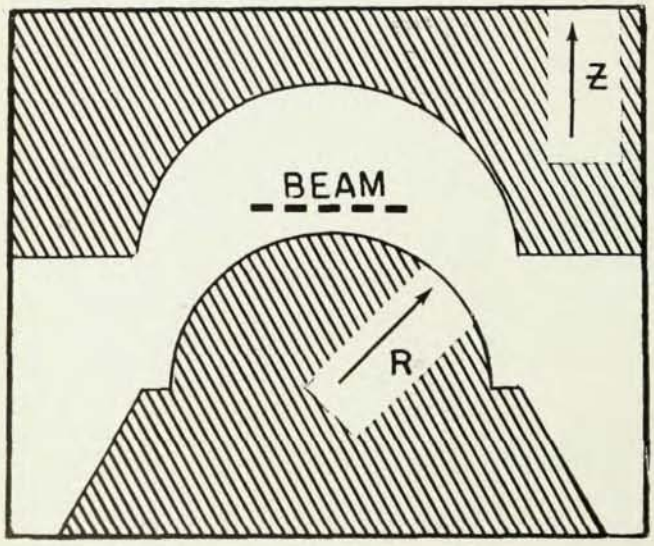


FIRST STAGE OF MOLECULAR BEAM APPARATUS. The beam path is shown here in the absence of magnets. —FIG. 3.

has not a transition occurred at a specified frequency?" A more subtle question does not ask for a yes-or-no answer but asks for the probability that a transition has occurred.

Rabi's Procedure

A beam of atoms or molecules issues from an oven in all directions, moves through a collimating slit, where the angular aperture of the beam is drastically reduced, and finally arrives at a detector. In figure 3 the beam that leaves S is a flat ribbon perpendicular to the plane of the paper. All this goes on in a very good vacuum. In the first apparatus the distance between oven and detector was 245 cm, largely because the apparatus was built out of bits and pieces left over from other research, and no attempt was made to optimize the system. The apparatus did demonstrate that the scheme was workable—not that there had been any significant doubts. For molecules, where the characteristic magnetic dipole mement is of the order of the nuclear magneton, μ_N, about $\mu_0/1836$, the distance might be of the order of 150 cm, and for atoms where the moment is of the order of μ_0 the distance can, in principle, be small, a few tens of centimeters. In any case, the whole thing goes on in a verg good vacuum to achieve long mean free paths.



POLES FOR RABI APPARATUS. Reversing curves, while retaining polarity, reverses gradient. —FIG. 4.

To describe the principle of the system we consider an atom with a single electron with a spin $s = 1/2$, outside of closed shells—that is, in a $^2S_{1/2}$ state. The nuclear spin is zero. No such atom exists in nature but the simplifying assumption is useful. Alternatively we can assume a diatomic molecule with no net orbital or spin electronic angular momentum and that contains one nucleus with a spin of 1/2 and another with a spin of 0. We will talk of the atom, but everything that is said is equally applicable to the molecule with some change in notation. The method, as first described in print on 15 Feb. 1938,[8] dealt only with molecules. Two years were to elapse before any work was done on atoms.

For the atom there is a single ground-state level, which splits into two components in the presence of a magnetic field. The magnetic quantum number of the electron is $m_s = +1/2$ or $-1/2$. The energy of the atom in a magnetic field is $W = m_s g_s \mu_0 H$. For the electron, $\mu_s = \mu_0$ to a good approximation, but the dipole moment is in a direction opposite to that of the angular momentum. Accordingly, $g_s = +2$ to an excellent approximation. The magnetic dipole moment of any system is $-dW/dH$. For $m_s = +1/2$, $\mu = -\mu_0$ and for $m_s = -1/2$, $\mu = +\mu_0$. This can be seen without a lot of manipulation. For, when $m_s = +1/2$, the angular momentum is parallel to the field, and since the dipole moment is oppositely directed to the field it is negative.

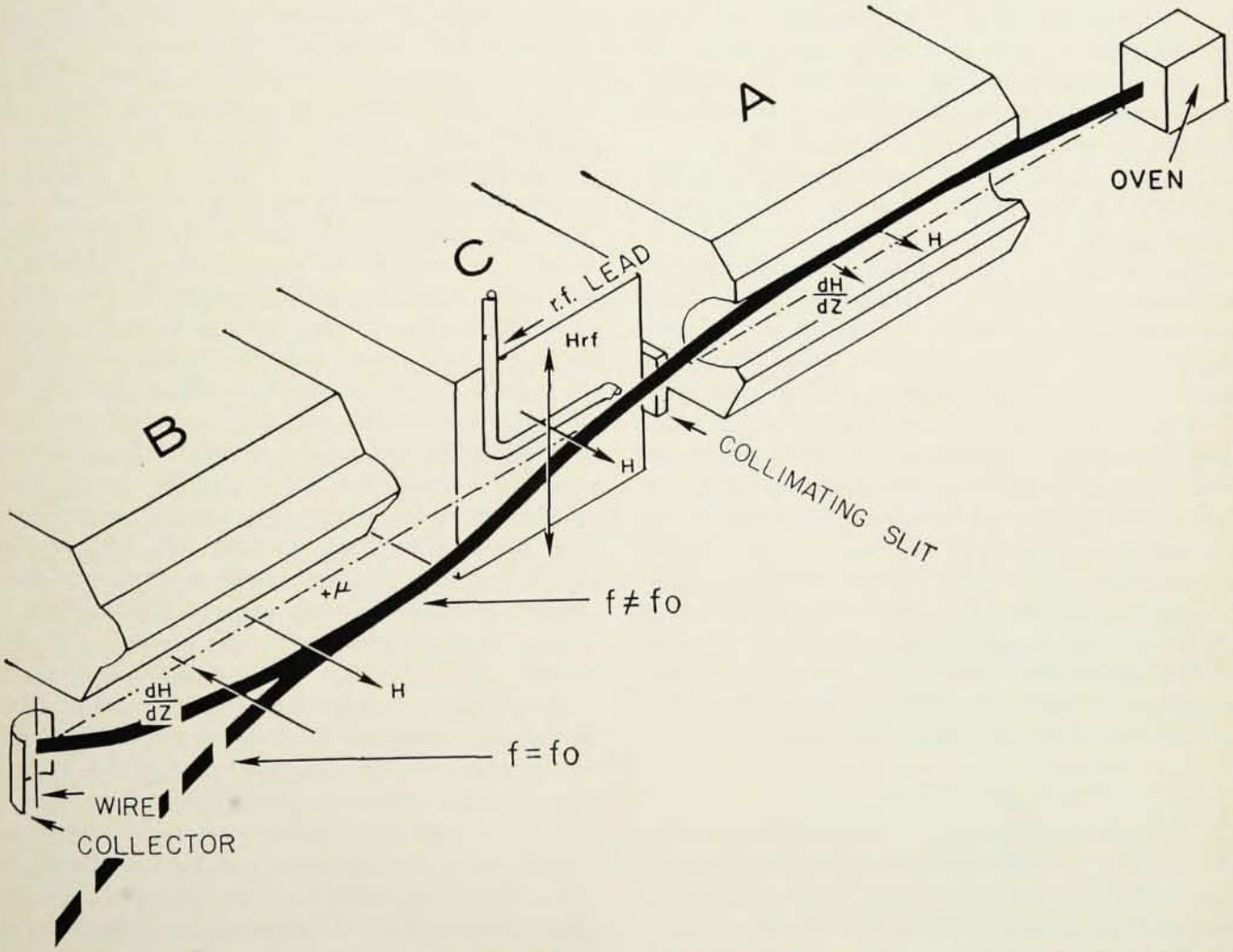
In region A of figure 3 we now place an inhomogeneous magnetic field whose pole faces are cylindrical and coincide with the magnetic equipotentials of two parallel wires carrying current in opposite directions.[9] A sketch of the magnet is shown in figure 4. The radius R may be as small as 1.25 mm and as large as 1 cm. Suppose the field points up. The gradient of the field, dH/dZ, points down. Since the force in the Z direction is $\mu\, dH/dZ$, all atoms in the beam with $m_s = +1/2$ will experience a force in the positive Z direction, and all those with $m_s = -1/2$ will experience a force in the

negative Z direction. If the direction of the gradient is reversed while the direction of the field is unchanged, the direction of the force is reversed. In the region B of figure 3 we do just that. By a suitable choice of gradient and lengths of various parts of the system, all atoms will be refocused on the detector independently of m_s and the velocity of the atoms (see figure 5).

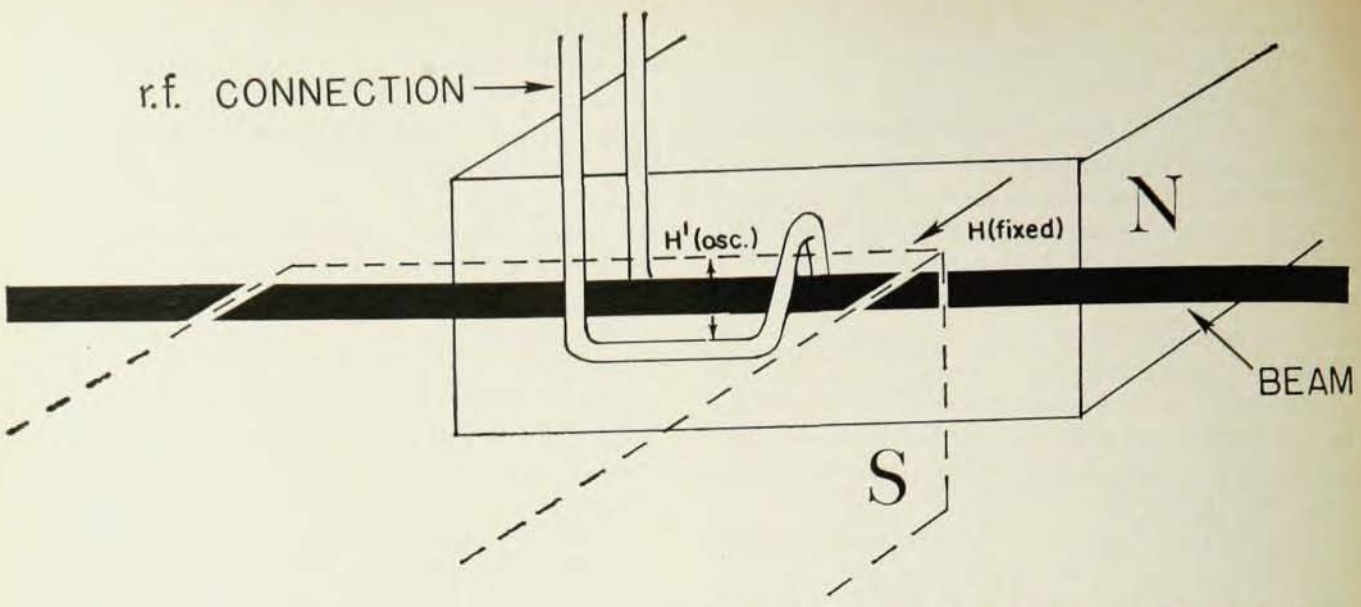
It is at once clear that if a transition were to occur between the states of $m_s = \frac{1}{2}$ and $m_s = -\frac{1}{2}$ anywhere in the region between A and B, the atoms would no longer strike the detector. Thanks to the exceedingly small energy difference between the two states, there are no spontaneous transitions from the higher state to the lower one.

The magnet C produces a homogeneous magnetic field whose value may be chosen quite independently of that in A and B. Its direction, however, is the same as that in A and B to preserve the space quantization of the atom. By applying an oscillating magnetic field of the appropriate amplitude, perpendicular to the constant field, and making its frequency equal to the energy difference between the two levels divided by h, a unit probability of transition from one state to the other can be achieved for atoms of a particular velocity. (See figure 6 overleaf.) If the intensity of the beam at the detector is measured as a function of frequency, a minimum will appear when the applied frequency $f_0 = \Delta W/h$. As a matter of fact, if the particular experiment that I am describing could be done, it would at once yield the spin magnetic moment of the electron—that is, $f_0 = g_s\mu_0 H/h$. The problem lies in getting a precise value of the experimental variable H and of the constant μ_0/h. Further, there is no atom precisely like the one I have described.

One should note that the dimensions along the beam are of the order of many tens of centimeters; the deflections perpendicular to the direction of propagation are of the order of many thousandths of a centimeter.



MOLECULAR-BEAM APPARATUS ACCORDING TO RABI. Beam path is shown as it occurs with the magnets on. If oscillating field in region C is of proper frequency to cause transitions, beam misses detector. —FIG. 5.



PRODUCTION OF OSCILLATING FIELD. Detail of region between C magnets in figure 5. —FIG. 6.

Once the basic idea that I have described gets across, one can dream up a large number of variations to meet different needs. The important ideas are that the occurrence of transitions can be detected by capitalizing on differential trajectories that atoms or molecules may make, depending on whether or not a transition has occurred; and that transitions can be induced by applying an oscillating field of appropriate polarization, amplitude and frequency.

Optical spectroscopy is limited in resolution by the lifetime of the excited state (in very many cases also by the Doppler effect). In the methods just described, all states involved have virtually infinite lifetimes. What is more, the Doppler effect can be eliminated to a very high accuracy. The width of resonance lines can be made arbitrarily small simply by subjecting the atom or molecule to the oscillating field for the time required by the uncertainty principle. This requires a relatively long time of flight in the C field; which, in turn, imposes increased demands for a high vacuum, increased mechanical rigidity of the apparatus and increased detector sensitivity because the intensity of the detected beam falls off inversely as the square of the distance between the source and detector.

Nuclear moments and hyperfine structure

In general the splittings of the ground states of atoms and molecules and the further splittings of the levels in a magnetic field (for molecules an electric-field splitting is also interesting) yield a whole array of data about nuclei: spins, magnetic dipole moments and electric quadrupole moments. The interaction of the nucleus with the enveloping electron configuration can also be found—the so-called "hyperfine structure." All these things can be found with a precision far beyond the wildest hopes of optical spectroscopists. It is like suddenly having a good microscope available to observe an organism that you had previously seen only with your unaided eyes.

The prewar work extended from 1937 to 1941, when all academic research in the United States came to a halt. In that interval this tremendously successful experimental enterprise did not attract a single imitator, to say nothing of a competitor. We could have coped with competitors for all kinds of reasons. But there were no imitators to offer the sincerest kind of flattery. This is an absolutely remarkable statement, quite unbelievable in the present environment. For a full year we, the members of the Columbia group, were the only scientists in the world who could systematically measure nuclear magnetic dipole moments—and, for all I know, we were the only ones in the universe. It was a wonderful environment in which to exist and work.

We had many successes during that period. First of all, we measured nuclear magnetic dipole moments. The very first one was[10] that of ^{7}Li—it turns out to have been a fortunate choice for a first attempt. All the initial measurements of nuclear moments were made through studies on molecules. The levels of real molecules have complicated structures but it didn't really matter. The big thing was that we could measure dipole moments, and if the resonance curves were not of the right width

as estimated by our rather ingenuous models, it wasn't really very relevant. The beauty of the method of determining nuclear magnetic dipole moments from observations on molecules was that the moment could be obtained from the raw experimental data by trivial calculation and very little dependence on theory. On the other hand, the inference of a nuclear magnetic moment from observed hyperfine structures required the injection of a considerable body of only approximate theory.

Not that we weren't concerned with an unexplained shape of resonance curves. The resonance curves were not only consistently too wide but they were also consistently asymmetrical. Sidney Millman, now director for physical research at Bell Telephone Laboratories and one of the heroes of the original work between 1937 and 1941, put his mind to the problem while he travelled on the IRT division of the New York Subway—and he came up with the answer. It turns out that the configuration of the oscillating magnetic field that we used was such that it gave an increased transition probability on one side or the other of the simple resonance curve, depending on the sign of the dipole moment. The asymmetry was not only *not* an unexplained nuisance, but it was a useful feature that allowed us to determine the sign of the moment. The effect is known as the Millman effect.[11] The discovery was fortuitous in the sense that it was made because the apparatus did not have an ideal structure.

There were important studies of molecular hydrogen in which the magnetic dipole moments of both the proton and deuteron were measured.[12] A really striking feature of the work was the discovery of the quadrupole moment (a departure from sphericity in the charge distribution) of the deuteron.[13] The experiments required the highest degree of virtuosity. The Taylor detector described earlier cannot be used to detect hydrogen. Rather, a Pirani gauge is used—a relatively insensitive device that won't respond elegantly unless you really love it.

In 1939 we started work on the hyperfine structures of atoms.[14] The hfs of all the readily available isotopes of all the alkali atoms were measured, as was the hyperfine structure of indium. Just before the war Jerrold Zacharias[15] measured the spin of ^{40}K, the very rare radioactive isotope of potassium. The importance of the work on ^{40}K is that it was an extension of the technique to allow the determination of the spins and dipole moments of extremely rare nuclear species. I should add that the spin of ^{40}K has a considerable intrinsic interest.

One of the last major papers published before the war was written by Millman and me and carried the title "The precision measurement of nuclear magnetic moments."[16] Theretofore the magnetic field in which a molecule was investigated and the frequency of the resonance were both measured. From these data and some fundamental constants a nuclear moment could be calculated; the problem was in the measurement of the magnetic field, at best a difficult experimental procedure, especially with high precision and in a vacuum. We reasoned that the need to measure a magnetic field could be avoided by observing in the same magnetic field a nuclear resonance and the electron-spin resonance. The ratio of the nuclear and spin resonance frequencies would at once yield the ratio of the nuclear and electron spin moments. Everyone, of course, knew that the electron spin moment was just μ_0. The experiment was a formidable one but did play a role, in spite of misconceptions, in the discovery of the anomalous electron spin moment.

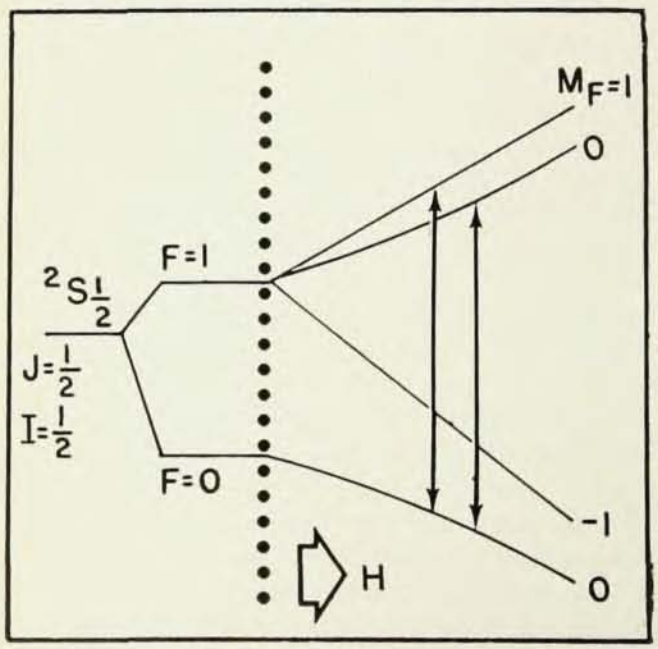
A molecule with no net electronic angular momentum but with a nucleus of spin I and moment μ_N has a resonance frequency $f_N = g_N\mu_0 H/h$ where $|g_N| = \mu_N/\mu_0 I$. An atom whose nucleus has a spin of zero with a single electron outside of closed shells—that is, an atom in a $^2S_{1/2}$ state—has a resonance frequency $f_s = g_s\mu_0 H/h$ where $|g_s| = 2\mu_s/\mu_0$. Even if the nuclear spin is not zero and hyperfine structure occurs, the quantity f_s can be extracted from the measurement of the frequencies of observable lines. Then $f_N/f_s = g_N/g_s$. If g_s is assumed to be 2, then g_N can be found. Precisely the experiment suggested by this reasoning was reported in the paper by Millman and me. The g_N values thus found were consistently about 0.12 percent greater than those found in earlier work, where only f_N was measured in a *measured H*. At the time this was a source of self-congratulation because it suggested that the field had in fact been measured to a considerably greater precision than we had any reasonable hope of achieving. When it later became known that g_s is greater than 2 by about 0.12 percent, I immediately returned to the old experiment. It turns out that the error in the magnetic field measurement was about 0.12 + 0.12 percent or about one quarter percent. For, had we used the correct value of g_s, the resulting value of g_N would have been still higher. Still, the accuracy in field measurement was commendable.

The war put a stop to all research that did not have a visible relevance to immediate needs. It would be a mistake to suppose that the lavish support that physics now gets was available immedi-

ately after the war; indeed, all apparatus had been dismantled and experimental physicists had to start from scratch with bits and pieces.

In any event I continued with work on hyperfine structures, and one of the first problems was the study, with Gordon Becker,[17] of the hfs of gallium. This admirable element is in a $^2P_{1/2}$ ground state but also exists in the metastable $^2P_{3/2}$ state, 826 cm^{-1} above the ground state. At a temperature of 1600°K about 50 percent of the atoms are in the higher state. This may be a dull subject but a knowledge of the properties of the atom in these two states was ultimately essential in the first measurement of the anomalous moment of the electron. The $^2P_{1/2}$ state of indium had been studied by Millman and Hardy just before the war.[18]

An exceedingly important experiment (on the hyperfine structure of hydrogen and deuterium) was done by Rabi and his students J. E. Nafe and E. B. Nelson immediately after the war. The importance of the measurement lies in the circumstance that the hyperfine splittings are presumably exactly calculable from known properties of the proton and electron and from fundamental constants. The ground $^2S_{1/2}$ state is split into two components through the magnetic interaction of the nuclear magnetic dipole moment with the magnetic field of the electron. The energy splitting between the levels of $F=1$ and $F=0$ corresponds to a frequency of about 1420.406 Mc/sec. The quantity is now known to a precision limited only by the accuracy with which the second itself is defined. In a magnetic field each of the levels has components whose energies depend on the magnetic field approximately as shown in figure 7. By measuring two transition frequencies at the same magnetic field (say $F=1, m=0 \leftrightarrow F=0, m=0$ and $F=1, m=1 \leftrightarrow F=0, m=0$) it is possible to deduce the zero field splitting. I should comment that the experiment was not an easy one because hydrogen is a tightly bound molecule and also because the detection of a hydrogen beam is difficult.



$^2S_{\frac{1}{2}}$ STATE OF HYDROGEN. Splittings and transitions under influence of magnetic field are shown. —FIG. 7.

In any case, Nafe and Nelson reported[19] a splitting of 1421.3 Mc/sec in their first preliminary report and later reported a value of 1420.410 ± 0.006. The striking thing is that the computed value was 1416.97 ± 0.54. The uncertainties in both the observed and computed values are of the wrong order of magnitude to suggest that the difference between the observed and computed values is other than real. One may look for the source of the discrepancy in a number of places, including the theory of the hyperfine structure and the values of fundamental constants. Gregory Breit[20] proposed that a possible origin of the discrepancy was a deviation of μ_s from μ_0. This would affect the calculated value of the hyperfine splitting in two ways. Suppose $\mu_s = \gamma\mu_0$, where γ is a constant. The proton moment μ_p interacts with the electron moment through the product $\mu_p\mu_s$. Millman and I essentially found the ratio μ_p/μ_s and gave μ_p under the assumption that $\mu_s = \mu_0$. The true μ_p is the published one times γ. The net effect is that the actual hyperfine splitting is up by a factor of γ^2 from that obtained by use of the current canon of physics. Breit's letter to the editor of *The Physical Review* was published in November 1947, but we at Columbia heard of it before then. Breit, like everyone else, obviously had a high regard for the body of atomic theory, and his suggestion had a kind of tentative quality about it—as though he was reluctant to make a suggestion in contravention to a body of knowledge that had, by and large, been thoroughly tested. These remarks in no way detract from Breit's imaginative suggestion.

Finding the anomalous electron moment

The work that I will now describe was done in collaboration with Henry Foley. It would have been impossible to avoid the interest of Rabi, his being what he is. Not that we didn't profit from it.

All the alkali atoms in their ground states have a single s electron outstide of closed shells and therefore occur in a $^2S_{1/2}$ state. Both gallium and indium have a single p electron outside of closed shells and subshells, and the state may be either $^2P_{1/2}$ or $^2P_{3/2}$. Associated with the total electronic angular momentum J, there is a g_J. In general

$g_J = \alpha_L g_L + \alpha_S g_S$. If Russell-Saunders coupling is assumed,

$$\alpha_L = [J(J+1) + L(L+1) - S(S+1)]/2J(J+1)$$

and

$$\alpha_S = [J(J+1) - L(L+1) + S(S+1)]/2J(J+1)$$

On all a priori grounds it is anticipated that the expressions above are, in fact, precise to better than one part in 10^4 for the particular atoms that we will consider. The various quantities of interest for the states in question are given in Table 1. Note that in all cases the angular momenta and dipole moments for the atom (in capital letters) are those of a single electron (in small letters).

Table 1.

State	S	L	J	α_L	α_S
$^2S_{1/2}$	1/2	0	1/2	0	1
$^2P_{1/2}$	1/2	1	1/2	4/3	−1/3
$^2P_{3/2}$	1/2	1	3/2	2/3	1/3

Suppose that $g_L = 1$ and $g_S = 2(1 + \delta_s)$, where δ_s is almost certainly small. Then,

$$g_J(^2S_{1/2})/g_J(^2P_{1/2}) = 3(1+2\delta_s)$$
$$g_J(^2P_{3/2})/g_J(^2P_{1/2}) = 2(1+3\delta_s/2)$$

Evidently a measurement of the ratios as indicated will yield a value of δ_s. If one could find atoms in the states in question whose nuclei have zero spin the problem might be simple. Actually we had studied the alkali atoms, gallium and indium, all of which are characterized by nuclear spin, magnetic dipole moment and hyperfine structure. In the $^2P_{3/2}$ state of gallium and indium the nuclear electric quadrupole moment complicates the hyperfine structure. We chose to study these particular atoms for the elementary reason that they could be detected very easily and highly selectively as compared to the large flux of residual molecules incident on the detector in the vacuum chamber. In any case all the relevant properties of the atoms in question that determine the energy levels of the atom in a magnetic field were known. The frequency of any transition between two levels always depends on the field only through the term $g_J\mu_0 H/h$. For example, the term $g_J\mu_0 H/h$ can aways be written in the stated form by a separate determination of g_I/g_J which is independent of any assumption about g_J.

The problem then is to measure the frequency of well chosen lines of atoms in each of two different states. From the measured frequencies and known constants it is then possible to find $g_J\mu_0 H/h$. Since our fields were not strictly constant in time, a long sequence of measurements was made in which lines of the two atomic species or states were alternately measured. I should note that the calculation of $g_J\mu_0 H/h$ was not a simple matter. Consider the $^2P_{3/2}$ state of gallium. $I = 3/2$ and $F = 3,2,1,0$. For the magnetic level $m_F = 0$, the four energies are the roots of a fourth order equation in $g_J\mu_0 H/h$. For $m_F = \pm 1$, the energies are the roots of two cubic equations. We describe a state by the notation (F,m_F). In measurements of the $^2P_{3/2}$ state of gallium we observed the lines $(3,0) \longleftrightarrow (3,-1)$, $(3,-1) \longleftrightarrow (3,-2)$, $(3,-3) \longleftrightarrow (3,-2)$ and $(3,-1) \longleftrightarrow (3,-2)$. For the first of these two cases the problem was to find a value of the parameter $g_J\mu_0 H/h$ such that the difference between the roots of two equations, of the fourth and third order, would be equal to an observed frequency. All this had to be done to high accuracy.

The result of the first experiment was published[21] in *The Physical Review* on 15 Dec. 1947. The letter has the rather curious title "Precision measurement of the ratio of the atomic g-values in the $^2P_{3/2}$ and $^2P_{1/2}$ states of gallium." It suggests the same kind of tentativeness in proposing a correction to the established canon of physical law that I detected in Breit's letter. Nevertheless, the letter does explicitly state that the departure of the ratio of $g_J(3/2)/g_J(1/2)$ from the accepted value of 2 could be explained by the assumption that $g_s = 2(1.00114)$. I am told that distinguished atomic physicists thought we were chasing a will-o-the-wisp, and not a very interesting one. How did we know that the g_J values computed by a previous expression were precisely correct?

Foley and I reported[22] (15 Feb. 1948) a measurement of the ratio of the g_J values of the $^2S_{1/2}$ state of sodium and the $^2P_{1/2}$ state of gallium. The departure of the ratio from the accepted value of 3 could be explained by assuming that $g_s = 2(1.00122)$.

The agreement between the value of g_s that would explain both observed ratios of g-values makes it unlikely that one can account for the effect by perturbation of the states. The departure of the g_J of sodium from g_s is presumably negligible. To explain our observed effect without modification of the conventional value of g_s introduces the unlikely requirement that both states of gallium be perturbed, and by amounts just great enough to give agreement as noted. We were sufficiently confident of the interpretation of the result to give the letter the title "On the intrinsic moment of the electron."[23]

The final results are in table 2 on page 34.

These results were universally accepted as demonstrating the existence of an anomalous magnetic moment of the electron and establishing its magnitude to good precision. It would be incredible if the perturbations on four distinct states were of

just the magnitude to give, in themselves, the agreement shown in the table.

In the same issue of *The Physical Review* that carried the second of the two letters, there was a letter by Julian Schwinger in which his newly [24] formulated quantum electrodynamics was used to obtain the result $g_s = 2\,(1 + \alpha/2\pi) = 2\,(1.00116)$.

Table 2.

Experimental Ratio	δs
$g_J\ (^2P_{3/2}\ \mathrm{Ga})/g_J\ (^2P_{1/2}\ \mathrm{Ga}) = 2(1.00172 \pm 0.00006)$	0.00114 ± 0.00004
$g_J\ (^2S_{1/2}\ \mathrm{Na})/g_J\ (^2P_{1/2}\ \mathrm{Ga}) = 3(1.00242 \pm 0.00006)$	0.00121 ± 0.00003
$g_J\ (^2S_{1/2}\ \mathrm{Na})/g_J\ (^2P_{1/2}\ \mathrm{In}) = 3(1.00243 \pm 0.00010)$	0.00121 ± 0.00005
	Av $= 0.00119 \pm 0.00005$

Herbert Taub and I had earlier started an experiment on the measurement of the proton moment in terms of μ_0. It was more or less an elaborate repetition of the prewar experiment by Millman and me. For complicated technical reasons we chose to measure the ratio of the g-value, g_p, of the proton in the sodium hydroxide molecule to the g_J of cesium in the $^2S_{1/2}$ state and that of indium in the $^2P_{1/2}$ state. The hyperfine structures of both cesium and indium were remeasured. We found [25]

$$g_p/g_J(\mathrm{In}) = 45.6877 \times 10^{-4}$$
$$g_p/g_J\ (\mathrm{Cs}) = 15.1911 \times 10^{-4}$$

There is much less reason to believe that g_J (Cs) $= g_s$ than that g_J (Na) $= g_s$, Accordingly we measured[26] the ratio $g_J(\mathrm{Cs})/g_J(\mathrm{Na})$ to be 1.000134. A combination of all these results yields

$$g_J(^2S_{1/2}\ \mathrm{Na})/g_J(^2P_{1/2}\ \mathrm{In}) = 3(1.00238)$$

This result is in excellent agreement with the previous one. I emphasize that the basic experiment is really identical to that by Foley and me. Still, it is worth while to do the same experiment, especially if it is important, in a variety of experimental frameworks.

The next phase of the work depends heavily on an experiment performed by J. H. Gardner and E. M. Purcell [27] in 1949. In the same magnetic field they measured the resonance frequency of the proton and the cyclotron frequency of the free electron. The first of these, $g_p\mu_0 H/h$, is measured in a spherical sample of mineral oil; the stipulation is an important one because there is a diamagnetic effect on the frequency determined by the nature of the medium and the shape of the sample. The cyclotron frequency of the electron is $2g_L\mu_0 H/h$. The mean value of $2g_L/g_p$ was found to be 657.4752 with a mean deviation in a large body of data of 0.0037 and a maximum deviation of 0.0056. The complete absence of systematic errors cannot be guaranteed (in spite of a careful study of possible sources of such errors). Gardner and Purcell state that the correct result is believed to be in the range.

$$2g_L/g_p = 657.475 \pm 0.008$$

The experiment was a most important one of considerable difficulty.

In 1950 Robert Karplus and Norman M. Kroll [28] published the result of a calculation of the electron moment to fourth order and found for the spin moment

$$\mu_s = \mu_0\ (1 + \alpha/2\pi - 2.973\ \alpha^2/\pi^2) = \mu_0\,(1.0011454)$$

A critical experimental test of the validity of this result subjects the procedures of quantum electrodynamics to test through one of the two directly measurable experimental quantities discussed by that theory. (The other is, of course, the Lamb Shift.) The test is of crucial importance because quantum electrodynamics is not a necessary consequence of other theory, but is a new intellectual construct in its own right.

Seymour Koenig and I set out to perform the experiment necessary for the test. [29] The problem was to measure g_s/g_p. Again we returned to that paragon of elements, hydrogen. It is the one element for which g_J can be precisely determined from g_s. In fact, the two differ only in a relativistic correction of 17.8×10^{-6} arising from an increase in the mass of the electron bound in the hydrogen atom. We therefore measured the resonance frequency of the proton (protons in mineral oil, as in the Gardner-Purcell experiment) in the same field as that in which we measured the frequencies of appropriate lines of hydrogen. From all these data we found the ratio $g_s/g_p = 652.2288 \pm 0.0006$. The combination of the new result with that of Gardner and Purcell gave $g_s/g_L = 2\,(1.001146 \pm 0.000012)$. The very large fraction of the uncertainty comes from the result of Gardner and Purcell. The agreement with the result of Karplus and Kroll is really splendid, and the only possible conclusion was that Gardner and Purcell had grossly overestimated the uncertainty of their result.

But flies began to breed in the ointment. Because of the large uncertainty in the experimental ratio g_s/g_L there had not been a really crucial test of the theory. A student of mine, Peter Franken, had worked on unrelated problems in the molecular

beam laboratory but was clearly interested in the problem. I have sometimes described him as an inspired lunatic. In any event he decided to repeat the Gardner-Purcell experiment at Stanford where he had his first academic appointment. He and his student Sidney Liebes found a value [30] for $2g_L/g_p$ that disagreed with the earlier result of Gardner and Purcell well beyond their experimental uncertainty and with a much smaller uncertainty of its own. They found that $2g_L/g_p = 657.462 \pm 0.003$, where g_p is again measured in a spherical sample of mineral oil. Liebes and Franken give the result

$$g_s/g_L = 2\,(1.001168 \pm 0.000005)$$

This obviously raised something of a crisis in physics, for the experimental result for g_s/g_L now disagreed with the theoretical prediction of 2 (1.001145).

Ultimately Charles M. Sommerfield [31] repeated the calculation of Karplus and Kroll and found that

$$\mu_s = \mu_0\ (1 + \alpha/2\pi - 0.328\ \alpha^2/\pi^2) = \mu_0\ (1.0011596)$$

The result is almost certainly correctly calculated and has the imprimatur of Karpus and Kroll. It turns out that the new experimental result agrees well with the result of the new calculations; Franken and Liebes, the most recent contributors to the result, express no concern with the difference between the experimental result of 1168 ± 5 and the theoretical 1160. After all, the ± 5 is an expression of a probable error and not of a possible extreme value. The result of Koenig and me has not been seriously challenged. Robert Beringer and Mark A. Heald [32] have found for $g_s g_p$ the value 658.2298 ± 0.0002, in good agreement with the earlier value of 658.2288 ± 0.0006. All this is an interesting commentary on the hazards of both experimental and theoretical physics.

Most recently D. T. Wilkinson and H. R. Crane[33] have made a direct measurement of the quantity a in the expression $g_s = 2\,(1 + a)$. The experiment is one of great beauty and ingenuity, performed directly on free electrons. It is not spectroscopic in character. Once again in the history of physics, a new technique may yield results of far greater precision than any visualizable extension of older techniques. It was found that

$$a = 0.001\ 159\ 622$$
$$\pm 0.000\ 000\ 027.$$

The experimental a can be described as

$$a = \frac{\alpha}{2\pi} - (0.327 \pm 0.005)\,\frac{\alpha^2}{\pi^2}$$

to be compared to the theoretical value,

$$a = \frac{\alpha}{2\pi} - (0.328)\frac{\alpha^2}{\pi^2}$$

where uncertainties in α have not been displayed in the uncertainty of the experimental result.

* * *

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